Amherst, Massachusetts
DEPARTMENT OF BIOLOGY

January 2, 1953

Dr. Bernard D. Davis
USPHS Tbc. Research Lab.
411 East Sixty-ninth Street
New York 21, New York

Dear Dr. Davis:

I found your letter here when I came up from Washington for Christmas. Perhaps you do not know that for a year and a half I have been the geneticist with the Division of Biology and Medicine of the Atomic Energy Commission. Therefore, participation on my part in our Salmonella studies has had to be reduced to fairly frequent week-end visits. However, the work is going on actively, and I think we are getting closer to a common ground with Zinder and Lederberg.

When we wrote our paper (P.N.A.S. 38, 797-803), Zinder's Jour. Bact. article was not out, and we were unaware that there were serious differences between our techniques and theirs. For instance, our strain which furnishes F.A. is lysogenic, and the recipient is not. Also we have no evidence whatever of a "stimulating" effect of another strain in producing F.A. from this strain. It appears to be a direct effect involved with the lysogenesis.

In addition, to attempt to find out if the differences in technique had any effect on the results, we have been repeating on an increased scale the tests of last spring reported in our paper. In this series we are checking somewhat more carefully the amino acids used and are retesting every isolated strain after successive transplants. We have done the same with some of the recombinants found last spring. This study is not yet completed, but we have already found that certain strains have changed. Specifically, there is no longer a single case remaining in which

F. A. has "carried" a requirement of the strain from which it was derived. It only causes reversion of one or several requirements to the wild type. Thus, we still get prototrophs from 1159 strain, requiring arginine, methionone, and aspartic acid, and also strains requiring two of these. Nevertheless, this strain did originally appear as a single step mutation.

I am sure you will see that I am just in the midst of revision of my ideas on this study, so it will be better for me to hold the strains until I can be sure of my own settled interpretations. Then, I shall be glad to send them along to you or to Lederberg directly if he wishes. Of course, I see the point you want to check, but our own work may well make such a re-test unnecessary.

At present I consider Zinder and Lederberg's interpretation of the phenomena unacceptable on two points:

1) Our tests are negative in respect to a stimulating effect of another strain in producing F. A.

2) I question on several grounds the idea that only one "character" at a time can be transduced.

But, after all, surely our conclusive independent confirmation of his over-all view is worth something.

I enclose our paper, along with a note which we sent to "Microbial Genetics Bulletin". Would you send me your two recent papers on Aromatic Biosynthesis.

Sometime I must stop in New York and talk these questions over with you.

With kind regards, I am

Sincerely,

H. H. Plough (J.K.)

HHP:JK